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The Myth of Research-based Practice: The Critical Case of Educational Inquiry

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Educational research in Britain has been subject to substantial government intervention in recent years. The rationale for this has been the demands made by a model of evidence-based policymaking and practice that carries implications for other fields of social research as well. In this paper, the growing influence of this model is sketched, and it is argued that it relies on some very questionable assumptions about what research can offer policymakers and practitioners. It is concluded that the consequences of enforcing this model are likely to be negative for both sides, but especially for research.

Currently, there is a crisis in the relationship between educational researchers, on the one hand, and policymakers and politicians, on the other. Of course, the same tensions are also to be found, increasingly, in other areas of social research, but in many ways educational inquiry is in the front line of these developments in Britain. And what is occurring in this area may well indicate what will happen in the future elsewhere, since the research-based policymaking and practice model that has stimulated this crisis has wide influence.1

The immediate origins of the current situation in educational research lie in the late 1990s. The most obvious sign of trouble was a lecture by David Hargreaves in 1996, sponsored by the Teacher Training Agency, in which he criticized educational research for failing to provide the kind of evidence that is required for evidence-based practice (Hargreaves, 1996). In making this critique, he held up as a model what he saw as the very different situation in medicine.2
Subsequent to Hargreaves’ lecture, both the Office for Standards in Education (OFSTED) and what was then the Department for Education and Employment (DfEE) set up inquiries into educational research, and these reported in 1998. The first, the Tooley Report, came to the conclusion that a substantial proportion of the published articles in educational research journals suffered from serious methodological defects. And while Tooley himself decided that most of these articles could be judged as educationally relevant in a broad sense, Chris Woodhead, then Chief Inspector and head of OFSTED, wrote a brief introduction to the report, in which he declared that much educational research is ‘on this analysis, at best no more than an irrelevance and a distraction’ (Tooley, 1998, p. 1). Furthermore, in the press release for the report (which was headed ‘Majority of academic educational research is second-rate’) he suggested that ‘considerable sums of public money are being pumped into research of dubious quality and little value’. The DfEE-sponsored Hillage Report also raised questions about the quality and, especially, about the usefulness of educational research, suggesting both that it needed to be more policy- and practice-relevant and that government ministers and policymakers needed to take more notice of research evidence (Hillage, Pearson, Anderson & Tamkin, 1998).

These two critical reports on educational research were followed by a government statement about what would be done to remedy the problems identified. In the words of Charles Clarke, then Parliamentary Under-Secretary of State at the DfEE, the aim was to ‘resurrect educational research in order to raise standards’ (Clarke, 1998, emphasis added); a comment which, to say the least, implies a rather negative view about the health of this field of inquiry.

In the wake of these developments, the government instituted a policy to reform educational research, of which there were a number of elements. One was establishment of the National Educational Research Forum, whose task is to facilitate identification of priorities for educational research, specification of quality standards in the field, and increasing the impact of research on policymaking and practice. Another initiative was the establishment of the Economic and Social Research Council’s (ESRC) Teaching and Learning Programme, which has come to constitute the bulk of externally funded research on education. Furthermore, a ‘research capacity building’ arm of this Programme has been set up at Cardiff, designed to up-skill education researchers, not least in the area of quantitative techniques. Finally, the EPPI-Centre (Evidence for Policy and Practice Information and Coordination Centre) was set up at the London Institute of Education in order to facilitate the production of systematic reviews in various fields of educational research. These reviews are intended to make the results of research available to policymakers and practitioners in a form that enables them to determine which pedagogical techniques or school policies are and are not effective, and thereby to improve the performance of the British education system.

What is significant for my purposes here is that a particular model of the proper nature of educational research is built into these developments. This requires research to focus on determining what ‘works’ and does not ‘work’ in education. And some see this function as implying that the ideal form of educational research is the randomized controlled trial, as in medical research (see Oakley, 2000). Such views have shaped the
filter by which studies are excluded from systematic reviews (Hammersley 2001). Furthermore, this focus on ‘what works’ is associated with a widespread and influential view about the socio-political function of both research and education: that one of their major roles, if not the primary one, is to facilitate national economic growth. As Alison Wolf points out in her book Does education matter?, this rests on a particular economic and political vision:

Politicians’ faith in education is fuelled by a set of clichés about the nature of the twenty-first-century world: globalized, competitive, experiencing ever faster rates of technical change. In this world, it seems, education is to be a precondition of economic success, and indeed survival, to an even greater degree than in the century before. (Wolf, 2002, p. xi)

This view is evident in the recent British Government white paper about universities (DfES, 2003). In his foreword, Charles Clarke, now Secretary of State for Education and Skills, writes that universities must ‘make better progress in harnessing knowledge to wealth creation’ and that they should be aiming to ‘turn ideas into successful businesses’. What we have here is an economistic view of the function of universities, and thereby of academic research.³

Explanations for Failure and the Emergence of the Research-based Model

Despite the unprecedented current level of government intervention in educational research, it is important to recognize that the relationship between this field and policymaking has always been problematic. While the term ‘evidence-based policymaking and practice’ is relatively new, the alleged lack of influence of educational research on policy and practice has been a recurrent theme of discussion for half a century or more.⁴ Typically, the explanations put forward for this problem fall into two main categories. Perhaps not surprisingly, these are to do with who gets blamed.

First, there are explanations which identify the problem as lying with researchers. The complaint is that educational research:

- is not closely enough focused on the concerns of policymakers or practitioners;
- fails to produce findings at the time they are needed;
- generates conflicting and therefore confusing evidence;
- provides evidence that is at odds with what is well known to policymakers and practitioners, so that its validity seems to be weak; and/or
- produces conclusions that are inaccessible to practitioners (because too elaborate and qualified, jargon-ridden, and/or published in journals that they do not read).

And such criticisms have, of course, by no means been limited to educational research; they are frequently directed against social research generally.

Often the remedy for this problem is seen as increased effort by researchers in disseminating their findings. Thus, emphasis is now placed by ESRC and other funders on researchers making contact with potential ‘users’ of their work, and specifying their proposed dissemination strategies in proposals. Sometimes, however, the problem is
M. Hammersley believed to lie deeper: in the very nature of the research that is done. This, for example, is the influential diagnosis offered by David Hargreaves in his Teacher Training Agency lecture (Hargreaves, 1996). From this point of view, the remedy requires potential users of research to be directly involved in setting the research agenda, and perhaps even directing or participating in the research process itself. Only in that way, it is argued, will research findings be produced that can be implemented in policy and practice.

The second type of explanation, by contrast, puts the blame on policymakers and practitioners, in other words, on the ‘users’ of research. In this explanation, practitioners—and especially policymakers—are portrayed:

- as closed-minded or set in their ways, and therefore as resistant to new perspectives;
- as committed to the dominant political ideology and unwilling even to consider radical challenges to it which research findings might imply;
- as untrained in the capacity to understand and make use of research; and/or
- as lacking in the motivation required to seek out research evidence and to reflect on their decisions in light of it.

In this spirit, not only policymakers but also teachers and other professionals may be blamed for being conservative, or sometimes even for being sexist or racist, in resisting what are seen as the practical implications of research findings.

Despite appearances, these two types of explanation are by no means incompatible. Implicitly, if not explicitly, criticism of the failure of research to inform policy and practice often attacks both sides simultaneously; though the emphasis usually falls on one or the other. Furthermore, the two explanations share in common the idea that if researchers and/or policymakers/practitioners were to behave in a rational fashion, then research would feed smoothly into policymaking and practice. And it is further assumed that the latter would thereby become much more effective. Standards would be ‘driven up’, to use the current jargon.

This is what I will refer to as the model of research-based policymaking or practice. In crude form, this portrays research as producing conclusions which can be made the basis for policies, whether at national level, at institutional level, or on the ground; policies that then lead to desirable outcomes because they are research-based. It is this idea that has led to calls on the part of the government for social and educational research to provide more evidence about which policies and practices work. An example is David Blunkett’s speech to the ESRC in early 2000. Here, Blunkett asked: ‘can the social science community have a major influence in improving government, or is it destined to be ever more detached and irrelevant to the real debates which affect people’s life chances?’ This was, of course, a rhetorical question.

Of course, researchers do not usually need much encouragement in wanting to make their research policy- or practice-relevant; even if what they mean by this is not always the same as what policymakers or practitioners have in mind. Educational researchers have often had strong commitments to particular policy ideas and practices, and believed that these ideas and practices could be justified by research findings. And, today, this applies both to those who support the present government’s policies and also to many who maintain a critical stance towards it.
However, I want to argue that the notion of research-based practice, which underlies both the types of explanation I have outlined, assumes too grand a role for research in relation to policymaking and practice. While high expectations may sometimes be a good thing, unreasonably high expectations are not; and those currently held of educational research fall into that category. Moreover, such excessive expectations are held not just by funders and potential users of research but also by many researchers themselves.

What I will argue here is that policy or practice cannot be based on research, in any exclusive sense, and that to try to make it research-based will distort either research or practice, or both. The most likely outcome, and the one that I am especially concerned with here, is a damaging effect on research. I should emphasize that I am not suggesting that research has no role to play in relation to policymaking and practice; simply that it cannot play the high profile role that the notion of research-based policymaking or practice implies; and that seeking to make it do so will have negative consequences. The result is likely to be:

- an increase in the likelihood of bias;
- further decline in the funds available for research that is not directly related to what are currently high priority policy issues;
- an increase in the amount of research which attempts to answer questions that simply cannot be answered effectively at the present time;
- a further reduction in the turn-around times demanded of research projects, which will make sustaining the quality of research work even more difficult than it is already.

In short, the eventual result of unreasonably high expectations will be disillusionment and recrimination; or simply the abuse of research as a means of supporting preferred policies and practices, or of discrediting disliked ones.

This is the background to my argument. Let me turn now to the reasons why I think that research cannot provide a basis for policymaking and practice, in the strong sense that currently seems to be assumed in influential quarters. Indeed, the points I will make suggest that the relationship between the two sides will never be a direct or a smooth one.8

Problems with the Research-based Policymaking and Practice Model

What I want to propose is that not only do researchers, policymakers, and practitioners not operate in the way that the research-based practice model assumes, but also that there are good reasons why they cannot or should not do so. The points I will make are summarized in Figure 1.

Research and the Needs of Policymakers and Practitioners

Let me start with the relationship between what research can produce and what policymakers and practitioners need. There are several points to be made here. First,
<table>
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<th>Research findings</th>
<th>must provide a basis for Practice so as to lead to Desirable outcomes</th>
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<td>BUT</td>
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<td>Research cannot provide all that practitioners need, they must draw on other sources of information and employ wisdom and judgement.</td>
<td>Research findings are always fallible, so that policies and practices drawing on them will not always lead to what is expected, and there may be unanticipated or unintended consequences.</td>
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<td>There is a difference between what researchers and practitioners take to be well-founded knowledge: organised scepticism versus a pragmatic orientation. This means that research will not provide what practitioners feel they need when they need it.</td>
<td>True factual knowledge is not sufficient for the success of a policy or practice. Indeed, false ideas can sometimes be more effective, and may even have desirable consequences.</td>
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<td>Practitioners will take account of other considerations than purely ‘technical’ ones, including what is and is not politically viable.</td>
<td>The impact of a policy or practice is always highly mediated by factors that are not under the practitioner’s control.</td>
</tr>
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<td>Research can provide too much or too detailed information, or the picture it provides may be too complex.</td>
<td>What is a desirable outcome is a matter of value judgement, and there will often be widespread disagreement about what is desirable, and especially about what is the highest priority.</td>
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<td>Research findings cannot in themselves tell us what is good or bad and what should be done.</td>
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**Figure 1** Some Problems with the Idea of Research-based Policymaking or Practice.
knowledge is not everything that practitioners need: they must necessarily rely on experience and judgement. So, in a literal sense, practice cannot be based on research, at least not exclusively. Furthermore, research could not ever supply even all of the information that practitioners need. The range of information and assumptions involved in most forms of occupational practice is very wide. There are certainly not enough resources available to researchers for them to cover any more than a small portion of this ground, nor are there ever likely to be.

However, the problem is not just a matter of scarcity of resources; it is more fundamental. There are likely to be significant differences between practitioners and researchers concerning the standards they use in accepting or rejecting knowledge claims; and perhaps with good reason. On some cogent accounts, social research modelled on science involves the requirement that, in determining what is and is not valid, researchers operate in such a way as to err on the side of rejecting what is true rather than of accepting as true what is in fact false. In other words, the emphasis is on avoiding false positives rather than false negatives. This is what is sometimes referred to as organized scepticism (see Merton, 1973, Chapter 13); and it is an approach that is necessary in order to maximize the likely validity of what are accepted as research findings.

By contrast, practitioners’ assessments do not usually have this consistent emphasis: what we might call the ‘acceptability threshold’ by which practitioners judge the validity of information typically varies according to practical circumstances. In particular, policymakers and practitioners are likely to be concerned with the relative costs of different types of error, and also with the extent to which specific errors are reversible (or remediable in some other way). Where the cost of error threatens to be high, or where it is irredeemable, the standards of assessment adopted will usually be such as to minimize this danger. However, where the likely cost of a particular kind of error is judged to be low, standards of assessment will not usually be designed to minimize it; avoidance of other possible errors will be given a higher priority. Moreover, this differential treatment of knowledge assumptions in terms of the likely costs of contrasting kinds of error is perfectly reasonable in the context of most forms of practice. While organized scepticism is the best policy in judging research, it is not a policy that anyone could actually live by: it would often leave us with no basis for making decisions that we are nevertheless forced to make because we cannot afford to delay them.

A related point is that the possible costs that are addressed by practitioners will not be solely to do with what would be the best technical or theoretical solution to a problem, but will also relate to what we might call the politics of the situations in which they operate. Some policy options will be seen as much more politically viable than others—even though they are not the best solutions in more abstract terms. Moreover, the politics of policymaking cannot be just wished away by imagining a world where such considerations would not operate, for example where policy would be made through deliberative democracy.

So, I am suggesting that there is a fundamental contrast in orientation between researchers and practitioners, and the result of this is that there will often be a mismatch between what the two groups treat as valid knowledge. Furthermore, there
are good reasons for this difference. One implication of this is that researchers’ criticisms of policymakers and practitioners are sometimes naive or utopian. Equally, practitioners’ criticisms of research often betray a failure to understand, or to respect, what is involved in coming to sound research conclusions. Another way of putting the point is as follows: what it is rational for researchers and for practitioners to take as sound knowledge will sometimes be sharply at odds.

It is also worth drawing attention to the fact that if research is governed by organized scepticism it will produce knowledge at a comparatively slow rate; and this is another reason why it often cannot supply practitioners with the knowledge they require. This is one reason why, even when knowledge relevant to practice is produced, it may not be sufficiently up-to-date to meet the needs of practitioners. In other words, it is in the very nature of research, or at least research of an academic or scientific kind, that it will often not be able to supply the knowledge that policymakers and practitioners feel they need at the time they need it.

The third main point, only superficially contradictory to the previous one, is that research may generate too much information about a particular topic—or information that is too detailed—for practical purposes. After all, researchers study particular issues in depth, whereas practitioners usually have to deal with a range of issues simultaneously, and often quite quickly.

It is worth noting that this is a respect in which policymakers and practitioners today differ from one another. Often policymakers, like researchers, focus on one issue at a time. But most practitioners on the ground do not have the luxury of doing this. Moreover, if practitioners were to take full account of all the relevant research findings available on all the issues relevant to their work, the result could be information overload, and this might well damage their work. In other words, it needs to be remembered that assimilating the information produced by research always takes time, and practitioners often work under great time pressure. So, we must take account of the fact that there are costs involved in making use of research information, and that sometimes these may reasonably be judged prohibitive.

Another, closely related, point is that research tends to complexify rather than to simplify; at the very least it complexifies before it simplifies. Thus, it often shows that the world is more complicated than practitioners think it is, that widely held stereotypes are false or only true in a very approximate way, that assumed causal relationships are more contingent than often supposed, and so on. Under some circumstances, showing practitioners the complexity and uncertainty of the phenomena they are dealing with, or revealing defects in their knowledge, can be beneficial. But recognizing complexity will not always be advantageous in practical terms. For instance, it may demotivate practitioners or dissuade them from taking any action at all on an issue.

The final point I want to make in this section is that research can only validate factual claims; it cannot in itself justify practical evaluations and recommendations. While we are often inclined to forget it, there is always the possibility of deriving quite different practical conclusions from any set of factual findings. Take the fact that boys underachieve in English by comparison with girls, in terms of their 16+ examination results. It might be concluded from this that:
• special remedial schemes must be introduced to improve boys’ performance;
• or that the way in which English is taught, or the nature of the English that is taught, discriminates against boys and must be changed;
• or that little can be done unless media and peer group images of masculinity are changed;
• or that nothing should be done because the differential performance of girls and boys in this area reflects an inherent tendency for girls to have greater facility with language, or the greater effort they put into learning. And, on this basis, it might be argued that taking steps to remedy the differential would be unfair on girls;
• or, finally, it might be concluded that nothing needs to be done because, for most pupils, once the basics of reading and writing have been acquired, English is not an important subject. Basic, functional language use is all that is necessary.

As this example indicates, what (if anything) should be done cannot be derived from any particular research finding in itself, but depends on other factual assumptions, and on evaluation of the information and of proposed remedies in terms of substantive (in this case, educational) values, as well as in terms of fairness and feasibility.

Since practitioners are primarily concerned with what is good or bad, acceptable or unacceptable, etc., and with what ought to be done in particular situations, the fact that research can only produce factual conclusions limits considerably the contribution that it can make to their work. Certainly, it cannot solve their practical problems on its own. In these terms, the notion of ‘applying’ research to (or even ‘translating’ it into) practice is just as misconceived as the idea of basing policy on research.

Policy into Practice

Let me turn now, more briefly, to the other linkage involved in the research-based practice model: that between research-based policies and desirable outcomes. It seems to be widely assumed that maximizing the impact of research on practice is always a good thing, that it will always produce good or better outcomes. This is evident in the frequent calls that are made on researchers to maximize the impact of their research findings. However, there are various reasons why this may not be desirable.

One is that all research findings are fallible. We are not faced with a contrast between well-established scientific knowledge, on the one hand, and mere irrational opinion, on the other; but rather with a dimension along which are ranged judgements having varying degrees of likely validity. Moreover, researchers have no guaranteed access to truth, and no monopoly on it. The fallibility of even natural scientific knowledge is now widely accepted. And how much more true is this of the knowledge produced by social research? There is a variety of reasons for this, one of which is the complexity of the phenomena social scientists study. I am not suggesting that the conclusions of social inquiry are completely unreliable, simply that their validity is never absolutely certain, and is often seriously questionable. And for this reason, if for no other, policy or practice relying on research evidence may not work—especially if this evidence comes from
a single study rather than from a whole body of work that has been thoroughly evaluated by the relevant research community.\textsuperscript{10}

However, on top of this, it is important to underline that a sound, evidential policy base is not all that is necessary for practical success. Indeed, it may not always be necessary at all: false assumptions can be effective—most obviously, but by no means exclusively, in propaganda or public relations terms. This is not to deny that, on balance, it is better to base policy on sound evidence, but it is to underline the fact that the relationship between the soundness of the research evidence on which a policy relies and its success is far from determinate. Policies will always involve many other assumptions that have not been researched and these will sometimes be as significant for the outcome as those that have been investigated.

A third important point about the relationship between policy and outcomes is that it is highly mediated. For one thing, the effects of any policy will often be significantly affected by local circumstances. Over and above this, in the case of national policy its implementation is by no means entirely under policymakers’ control. As many writers have pointed out, national policies have a trajectory that is highly contingent, mediated by various agencies along the way, so that what a policy looks like on the ground may be very different from what the policymakers originally envisaged (see, for example, Ball, 1990). And, as a result, the outcomes may not be at all what was intended.

This relates to a more general issue: about the limits to what anyone can do to improve a situation. Improvements are not always possible, they are rarely cost-free, and sometimes the cost outweighs the likely improvement. Moreover, not every problem can be solved, and attempts to improve matters can sometimes worsen the situation. For these reasons, a ‘can-do’ attitude does not always lead to significant, let alone desirable, change. There are severe limits to the effectiveness of all policies—the world is too diverse and uncertain a place for policy success to be guaranteed. And the more ambitious policies are, the more likely they are to fail, or to have serious unintended consequences. There is a danger that research comes to be regarded as providing more confidence than is justified in the likely success of policies that appeal to its findings for support.

The final point in this section is that what is a desirable outcome is a matter of judgement, and there will not necessarily be consensus about this. The research-based policymaking and practice model tends to assume that there is agreement about what would count as a good outcome when (often) there is not. Potential disagreement is covered over by talk of, for instance, ‘effective schooling’ or ‘effective teaching’. After all, who could be in favour of ineffective teaching? But this is to ignore the plurality of educational values to which people are committed, and the different priorities that can be adopted amongst these. Furthermore, policies always have multiple consequences, some of which may be judged beneficial, others not. For all these reasons, the issue of whether the results of a policy or practice are desirable is not as easy to determine as the model implies.

Underlying many of the points I have made here is an emphasis on the fact that social scientific research and policymaking or practice are activities that are very different in character from one another. This may seem an obvious point, but it is one that
tends to be overlooked by some of those on both sides of the divide. For example, policymakers tend to see educational research as analogous to the collation of information from different sources that civil servants engage in when responding to a minister’s request for background information. It is by comparison with this image that they often judge academic work to be insufficiently issue-focused, overly complex, and far too slow.

By contrast, researchers frequently look on policymaking and practice as if they were or ought to be similar in character to academic research. An example is the charge that policymakers are closed-minded, this implying a contrast with the open-mindedness that is supposed to be characteristic of researchers. However, it tends to be forgotten that a degree of closed-mindedness is essential to get anything done (research included). And it is certainly true that practical decisions often have to be made before all the information is available that would be judged necessary from an academic point of view.

Let me emphasize, once again, that I am not arguing that research is of no value for policymaking and practice, simply that it is of less determinate and less predictable value than the research-based practice model assumes. Of course, what I have done in this article is to complexify the relationship between research and practice; and, on my own account here, this is what one would expect from a researcher. But I believe that in this context it is very important to be realistic about the nature of that relationship.

What is Meant by ‘Research’ in the Research-based Policy Model?

Let me end by noting that the policy that the government is currently applying to educational research is itself presented as an example of research-based policymaking. Government ministers sometimes point back to the Hillage Report as the basis for their intervention in educational research, for example their decision to set up an Educational Research Forum to coordinate the research priorities of researchers and funding agencies.

Now, this point carries an interesting implication. It makes the Hillage Report itself a model for the kind of educational research which the government sees as required if education policy and practice are to become evidence-based. And what is striking about the Hillage Report is that it was quite similar in character to what I referred to earlier as the civil service model: the collation of information to address some current issue.

The Hillage team had only a few months to do their work. They canvassed opinion from a range of stakeholders, and drew on documentary evidence, summarizing what they took to be the main conclusions that could be drawn, and framing recommendations for government policy. And, in doing so, they made a variety of assumptions that are open to serious question. For example, they blur the distinction between academic research on education and research of the kind they are themselves engaged in, which is designed directly to inform a policy decision. While they accept the value of academic research, the trend of their proposals leaves little or no room, and by implication very few resources, for it.
They also assume that they can rely on their informants’ opinions about the relevance and impact of particular pieces of educational research, or of educational research in general; as if these were matters that anybody could observe in a non-inferential and incontestable way. Yet, if there is a single finding that comes out of all the research that has been done on research utilization it is that direct and demonstrable impact is rare, while more indirect, difficult to detect, and subtle influence is widespread. A key reference here is the work of Carol Weiss (Weiss, 1977; Weiss & Bucuvalas, 1980).

A final feature of the Hillage Report is worth noting, in its role as a model for future practice-relevant educational research. This is that its conclusions seem to have predated the carrying out of the research: indeed they were referred to in the press reports that announced the setting up of the Hillage investigation. For instance, Michael Barber, who was at that time head of the Standards and Effectiveness Unit at the then Department for Education and Employment, was quoted as saying: ‘It is extremely unlikely that the status quo will survive this review’. And, in fact, the idea of an Educational Research Forum had already been suggested by David Hargreaves long before the Hillage inquiry was set up (Hargreaves, 1996).

This might seem to confirm the cynical view of many researchers that the way to ensure that one’s research has an impact is to tell policymakers and practitioners what they are already thinking, so that they can then claim that what they are proposing is research-based. While I do not want to be too cynical about this—I have argued in this article that there are good reasons why policy and practice ought not to be research-led—nevertheless there is a current danger of research being turned into an ideological tool by policymakers. Evidence-based policymaking or practice is one of those ambiguous symbols that people of diverse views can appeal to when they actually mean very different things by it: it is Janus-faced.

Conclusion

To a large extent, what has happened to educational research is that it has become subject to the ‘new public management’ (Clarke & Newman, 1997; Pollitt, 1990; Power, 1997). And similar pressures are being felt in other areas of social inquiry as well. However, it is not just that research is now to be rendered transparently accountable. The assumption behind evidence-based policymaking and practice is that research can play a key role in rendering the public sector as a whole more accountable. So, the aim has been to reshape educational research so that it can serve this function more effectively, and to set up regulatory procedures to control its performance of that role.

In my view, this is a role that educational, and other, researchers should resolutely refuse. This is because the notion of research-based policymaking and practice is a myth. Of course, it may be a myth that many researchers feel it is in their interests to preserve. But they are wrong. The medium term, and perhaps even the short term, consequences of this myth are likely to be damaging, not only for research but also for policy and practice.
Acknowledgements

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Notes

[1] My main focus in this paper will be on academic research carried out in universities. Elsewhere, I have tried to clarify the different forms that social research can take, in terms of distinct relationships to policymaking and practice (Hammersley, 2000) and the issues I discuss here will clearly vary in significance and character for these different forms. However, I believe that many of the central points are relevant to all kinds of social research. For further elaboration of the arguments in this paper, see Hammersley (2002).


[4] Nisbet and Broadfoot (1980) have provided a useful account of this history.

[5] This was true of the Hillage Report, for example (Hillage et al., 1998).

[6] The term ‘evidence-based’ policymaking or practice is perhaps more common, but in most usages the reference is to research evidence. There have also been reformulations that downplay the role of evidence; an example is ‘evidence-informed’. However, in practice usage of this term does not seem to signal any major alteration in view.

[7] Or, to give it its correct name, an ‘interrogatio’; see Dixon (1971, p. 36).

[8] I do not make much of a distinction here between policymaking and practice, and this is potentially misleading. Elsewhere, I have argued that the attempt to gear educational research into supporting evidence-based practice is part of a managerialist form of policymaking which is concerned with ‘reforming’ educational practice, in line with the economistic ideology that I outlined earlier (see Hammersley, 2002, 2004a). There is a triangular relationship involved, not just a two-sided one. However, for the purposes of what I want to say in the remainder of the paper, this is not of great significance.

[9] That there are costs associated with the search for and use of information has long been recognized by economists (see Lamberton, 1971).

[10] For this reason, the current emphasis on producing reviews of the research literature for use by policymakers and practitioners is to be applauded. Unfortunately, though, this has often involved a conception of research synthesis that involves misconceptions about the nature of research and its relationship to policymaking and practice. For these arguments, see Hammersley (2001, 2004b).

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